theoretical calculations is more than satisfactory; especially in view of the difficulty of making, in a material soft and easily distorted, the cuts called for by the theory.

It would certainly be preferable to make use of celluloid, as Prof. Coker now does. I did indeed attempt it at the time of these experiments, but experienced some little difficulty in glueing the celluloid after cutting it, inasmuch as, in accordance with Volterra's theory, pressure is exerted on some regions of the faces in contact, tension on others.

The optical method, which permits the investigation of the internal distribution of tensions, has thus given us the means of verifying the theory of elasticity in a salient point, namely Volterra's theory of distortions, just as it has given Prof. Coker the means of supplying, by the use of experimental models, certain deficiencies of the theory—a theory which cannot always submit to rigorous calculation the complex conditions of internal strains to which the materials of practical construction are subject.

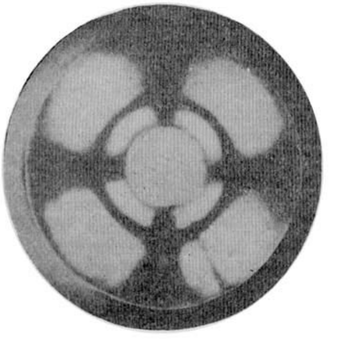
FIG. 3.

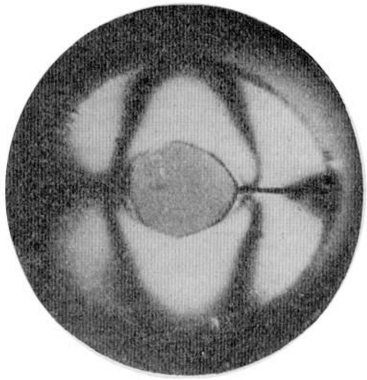
FIG. 4.

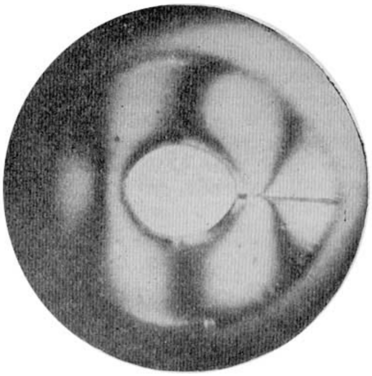
FIG. 5.

O. M. CORBINO.

Rome, Physical Institute of the Royal University.

The Bacterial Theory of Soil Fertility.

IN vol. v., part i. (October, 1912) of *The Journal of Agricultural Science,* Messrs. Russell and Petherbridge state that "partial sterilisation appears to be the proper method of dealing with 'sick soils'" (p. 91). I venture to think that the theory of *partial sterilisation*—which is indeed very attractive—will not stand examination. According to this theory the fertility of a soil depends largely on its bacterial population, the enemies of which are destroyed by partial sterilisation, which the bacterial spores survive.

Now if this theory is correct, it should follow that complete sterilisation must diminish the fertility of a soil, since all bacterial spores will have been destroyed. This, however, is not the case, as Dr. Russell is apparently aware, for Drs. Darbishire and Russell in the same journal (vol. ii., part iii., December, 1907, p 319) state: "a few experiments have been made with soils heated to 120° C. The same kind of results are obtained as at the lower temperatures, but they are somewhat intensified."

In other words, complete sterilisation gave an increased crop over partial sterilisation. The comparative effects of complete and partial sterilisation on a soil were shown by the present writer (*Cairo Sci. Jour.,* vol. iv., No. 43, April, 1910), maize in soil untreated, soil heated to 95° C., and soil heated to 170° C. yielding green weights in the proportion of 145·5, 151·7, 1055·6 (see Fig. 1).

Without having experimented on the effects of complete sterilisation of a sick soil, Messrs. Russell and Petherbridge state (*loc. cit.,* p. 90), "our experiments thus lead to the conclusion that at least two factors are concerned in soil sickness: a falling off in bacterial activity and an accumulation of plant parasites and disease organisms."

As a matter of fact, all that is proved is, as in Drs. Russell and Darbishire's paper quoted, that partial sterilisation produces both increased crop and increased bacterial activity. The illogical conclusion is then drawn from this that increased crop is due to increased bacterial activity. This erroneous deduction would not have been arrived at had a few parallel experiments been conducted with completely sterilised soils.

Again, the authors find that plants grow as well in extracts from the "sick" soil as in extracts from partially sterilised soil. They conclude that the "sickness" is therefore not due to a soluble toxic substance. But does this prove it? The phenomenon of absorption (or adsorption) of soluble salts by soils appears to have been overlooked. Further, these results are in direct contradiction to the very elaborate experiments carried out by the U.S.A. Department of Agriculture.

With regard to the growth of seedlings in water extracts of soils, very little detail of the method of the experiments is given. For instance, it is not stated at what stage the seedling was planted in the water extract. That certain precautions may have been overlooked would appear possible from the statement (used as an argument against the toxic theory) that "cucumber seeds are very sensitive to unfavourable conditions, but they germinate fully as well in 'sick' soil as in partially sterilised soil."

Now Pickering (*Journ. Agric. Sci.,* vol. ii., part iv., and vol. iii., parts i. and iii.) pointed out that germination is delayed in heated soils—a fact long known to farmers—and supposed that this was due to the production by heat of a toxic substance. The present writer (*loc. cit.*) proved that this delay is due entirely to a physical cause, viz. the increased osmotic pressure in the water contents (and water extract) of a heated soil; this causes imbibition by the seeds to be checked, in some cases to such an extent that they rot before they have absorbed sufficient water to cause germination.

Now if the seeds in Russell and Petherbridge's experiments were germinated in the soil extracts, after five days' growth (plate iii., Fig. 3*a*), we should scarcely expect the seedling in the extract from the heated soil to have made up for time lost in germination. Even if the seeds were all germinated under the same conditions, *e.g.* in water, and seed-

© 1913 Nature Publishing Group

lings in similar stages of growth were then transferred to the soil extracts—as was the case in the writer's experiment above quoted—we still have in the case of some varieties of plants this delayed germination period extended to a considerable length in water cultures; in other words, the extract from a heated soil retards growth during a period of several days after the germination has actually taken place. This appears to be connected in some way with the formation of root hairs, the growth of which is often entirely inhibited in water cultures.

The safest way of testing the effect of various soil extracts on plant growth is to sow seeds in different portions of one and the same soil (which should not be too rich), and then, after germination, water with the various soil extracts.

Until some assurance is forthcoming that the necessary precautions have been taken, the results of the water cultures mentioned cannot be accepted.

Finally, it will be difficult for any theory of soil fertility that, like Dr. Russell's, claims that all, or almost all, depends on bacterial activity to explain away the injurious effect of any one plant on all its neighbours (see present writer in *Journ. Agric. Sci.*, vol iv., part iii.).

F. FLETCHER.

Kyambu, British East Africa, November 27, 1912.

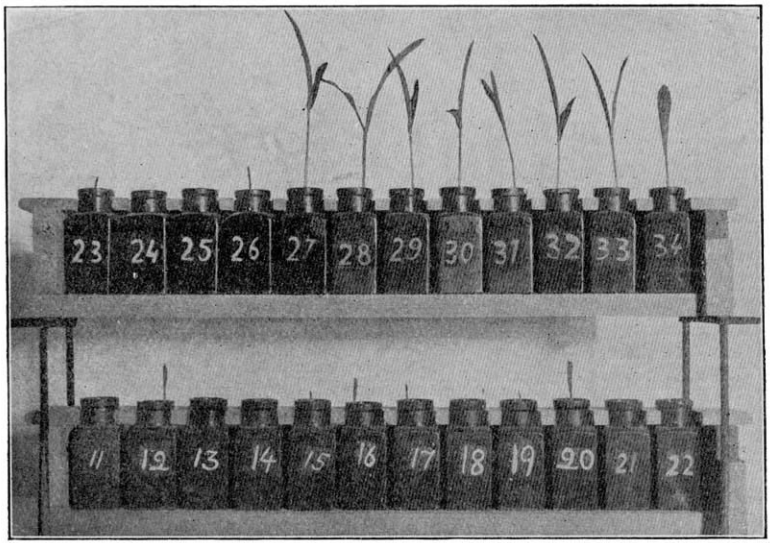

FIG. 1.—Maize plants after growing for seven days in soil previously treated as follows:—11–18 not heated; 19–26 heated to 95° C.; 27–34 heated to 170° C.

MR. FLETCHER is under a misapprehension in attributing to me a "theory" that "all, or almost all," of soil fertility depends on bacterial activity. I should certainly agree with Mr. Fletcher that any such hypothesis, if it were advanced, would be much too narrow to account for the facts.

Soil fertility is not due to the operation of any one factor, but of several. At least five conditions have to be fulfilled by the soil if the plant is to make satisfactory growth. There must be (1) adequate food supply; (2) proper water supply; (3) suitable temperature; (4) enough air for the roots; (5) absence of injurious substances or factors. Every one of these conditions is essential; any one that is unfulfilled sets a limit to the growth of the plant, and therefore to the fertility of the soil.

I have discussed the interaction of these various factors at some length in my book on "Soil Conditions and Plant Growth," and need now only refer to the place of bacterial action in the scheme.

Among the various nutrients required by the plant are the nitrogen compounds. Nitrates are the compounds usually obtained from the soil, but ammonium salts also serve; there is evidence, however, that highly complex compounds like the proteins, peptones, &c., are of little value to the plant even when they are soluble. Now the nitrogen compounds of the soil are mainly complex and insoluble, but they decompose slowly to form ammonia, which then oxidises to nitrates.

It has been repeatedly demonstrated that *when all the other essential conditions are satisfied,* an increase in the supply of ammonium compounds or of nitrates increases the amount of plant growth, *i.e.* of soil fertility. An increased supply of ammonium salts and nitrates may be brought about either by direct addition of these compounds or of substances easily converted into them, or by increasing the rate at which ammonia production takes place in the soil.

The production of ammonia in the soil is largely due to bacteria. When the conditions are made more favourable to bacterial action a marked increase in activity sets in, accompanied by an increased production of ammonia and nitrate. A corresponding increase in soil fertility follows. Partial sterilisation of the soil leads to marked increases in bacterial numbers for reasons that Dr. Hutchinson and I have discussed elsewhere. The accompanying increase in the amount of ammonia produced is so closely connected with that of the bacterial numbers that no reasonable doubt can be entertained as to its bacterial origin.

So much for the general relationship of bacterial activity to soil fertility. We can now turn to some of the details raised by Mr. Fletcher. He goes on to say that if bacterial activity has anything to do with soil fertility a completely sterilised soil ought to be less fertile than a partially sterilised soil. Unfortunately no one has ever succeeded in carrying out this experiment. When a soil is heated to 170° C., as in Mr. Fletcher's experiments, or even to 120° C., as in some of ours, it alters so completely that it can no longer be compared in any sense with the unheated soil. A considerable amount of decomposition takes place, and much ammonium and other simple soluble nitrogen compounds are formed. There is no reason to suppose that it matters to the plant whether the ammonium and other compounds are formed by bacterial action or by any other process; the essential point is that they should be formed; whatever their origin, they serve as plant nutrients. The increased gain in plant growth on such highly heated soils can be largely attributed to this cause.

The water-culture experiments, like the other experiments made at Rothamsted, were carried out with all the care and precautions that we could command. The obvious pitfalls mentioned by Mr. Fletcher were avoided. The fact that our results differ from those obtained by the United States Bureau of Soils implies no contradiction at all; they worked with "sour" soils, and we worked with the entirely different "sick" soils. We could find no evidence whatsoever of the presence of any toxin in our sick soils, or in our

© 1913 Nature Publishing Group